MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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WILLIAM LASSELL, Esq., F.R.S., President, in the Chair.

William Godward, Esq., 5 Clarence Road, Finsbury Park ;
William Bath Kemshead, M.A., Ph.D., 53 Knowle Park, Brixton ;
Frederick J. Marriott, Esq., Perry Hill, Sydenham ;
Charles Frederick Plant, Esq., Sandfield House, Mount Vernon, Nottingham ;
Chintamaney Ragoonathachary, Madras Observatory; and
George Kinneard Winter, Esq., Telegraph Engineer to the Madras Railway,

were balloted for and duly elected Fellows of the Society.

THE TOTAL SOLAR ECLIPSE OF 12 DEC. 1871.

Letter from M. Janssen to the President.

J'aurai l'honneur d'adresser à la Société Royale Astronomique un mémoire détaillé de mes observations de l'Eclipse, mais je profite du départ de ce courrier pour vous informer des principaux résultats obtenus.

Sans entrer dans une discussion qui fera partie de ma relation, je dirai d'abord que la magnifique couronne observée à Sholoor s'est montrée sous un aspect tel qu'il me paraîtrait impossible d'admettre ici une cause de l'ordre des phénomènes, ou de diffraction, ou de réflexion sur le globe lunaire, ou encore de simple illumination de l'atmosphère terrestre.

Mais les raisons qui militent en faveur d'une cause objective et circumsolaire prennent une force invincible quand on interroge les élémens lumineux du phénomène.

En effet, le spectre de la couronne s'est montré dans mon télescope, non pas continu comme on l'avait trouvé jusqu'ici, mais remarquablement complexe. J'y ai constaté :

Les raies brillantes, quoique bien plus faibles, du gaz hydrogène qui forme le principal élément des protubérances et de la Chromosphère ;

La raie brillante verte qui a déjà été signalée en 1869 et 1870, et quelques autres plus faibles ;

Des raies obscures du spectre solaire ordinaire, notamment celle du sodium (D.) Ces raies sont bien plus difficiles à apercevoir.

Ces faits prouvent l'existence de matière dans le voisinage du Soleil, matière qui se manifeste dans les éclipses totales par des phénomènes d'émission, d'absorption, et de polarisation.

Mais la discussion des faits nous conduit plus loin encore.

Outre la matière cosmique, indépendante du Soleil, qui doit exister dans le voisinage de cet astre, les observations démontrent l'existence d'une atmosphère étendue, excessivement rare, à base d'hydrogène, s'étendant beaucoup au de-là de la Chromosphère et des protubérances, et s'alimentant de la matière même de celles-ci, matière lancée avec tant de violence à travers la photosphère, ainsi que nous le constatons tous les jours.

La rareté de cette atmosphère, à une certaine distance de la Chromosphère, doit être excessive ; son existence n'est donc point en désaccord avec les observations de quelques passages de comètes près du Soleil.

Neelgherry, Sholoor, 19 Décembre, 1871.

Letter from Col. Tennant to Dr. Huggins.

At 8 A.M. I sent you the following telegram :—

"Thin mist, spectroscope satisfactory. Reversion of lines entirely confirmed. Six good photographs."

I have now just come down the hill, leaving my able assistants to dismantle ; and I hasten to give you a sketch of what has been done, to go by to-night's mail.

At $19^h\ 31^m\ 50^s$ I saw a projection from the Moon's cusps, and on this set the slit of the spectroscope. Herschel saw *many lines*. He wished to note, but by a misapprehension I did not give him the signal till some twenty seconds later, when he got a few from the rapidly moving cusp which I was following. This ended at the apparent top of the Moon by the slit passing through a prominence.

The lines appeared rapidly one after another, and I believe it would have been impossible to record them or to identify more than one or two previously marked with pointers.

I then, by agreement, came to the centre of the Moon, which was reported blank. We placed the slit across the edge of rift whose position (that of the edge) I estimated at 120° from the vertex. It was about 10′ from the Moon, and K 1474 was, I believe, the only result in lines, and it seemed to cross the field; but to this point sufficient attention unfortunately was not paid, from my having omitted to tell Herschel what he was looking at. I then turned to a thin rift truly above the Sun, and some 8′ away from the Moon. K 1474 was again only the result, though in both cases Herschel seems to have seen *traces* of lines. I then about doubled the distance from the Moon. I had still light, and saw the rift; but Herschel had no lines. Finally, I approached the rapidly coming edge of the Sun, and lines began to come quickly, but the Sun was out before any could be recorded.

I have only had a conversation with Herschel, and he cannot see this before it goes and correct it, so that I may be wrong in some detail, but, I believe, this is what he reported.

For myself I was using the Directing Telescope on what we have unanimously called the Twins. I was intent on following the cusps for Herschel, and thus lost the opportunity of examining the polarization of the light which first threw out the whole Moon. I had my attention specially directed to the chromosphere and prominences at the true bottom of the Moon's limb. They first appeared *white*, and then changed through pink to red. I am quite confident that I saw no blue or green tinge. You will remember my suspicion that I do not readily see blue when faint, but I am sure the tint was not strong. My waiting in the Moon's centre gave me an opportunity of looking about me, and I saw the two rifts I have spoken of. To me they did not reach the Moon's edge, but were separated by some minutes of bright corona. The corona's outer layers were undoubtedly radiated, but not coloured. There were alternate gradations of light; that is, light and comparative darkness, but certainly no colour. The rays were lost as they neared the Moon. The rift at the true vertex of the Sun I am sure did not change, nor I believe did any other. Colonel Saxton had made for my object-glass a loose cap of pasteboard, with a two and a half inch aperture to be used during the Sun's presence; this was to have been knocked off just before totality. I forgot this, but I doubt if I should have improved matters; I used the power 35.

I had asked Captain Morant, R.E. (to whom I am indebted for a very great amount of assistance), to take a very beautiful reconnoitring telescope by Dallmeyer, which was mounted on a stand. Of 14 inches focus, it has also power (15) and an aperture of 1·75 inches. He (Captain Morant) made two sketches of the corona, which generally confirm, as does his description, all I have described. He expressed himself positive as to the absence of colour and the permanence of rifts, &c., in certain positions (not mine), and only differed in thinking that the shades

were of a sepia tinge (brownish) and that the rifts did extend to the Moon.

Now to the photographs. We have had some glorious mornings. Yesterday Herschel and I were up at *η Argûs*, but had no great amount of examination, as our intention was to focus slit, &c. on stars. Though the evenings have been misty, at times the mornings have been very clear, and we hoped they would remain so. Last night was very clear, and continued so till about $2^h 30^m$ a.m., when it became cloudy, and afterwards rained slightly. By the time for getting up it was less thick, and rain had ceased, but a wind had sprung up from the S.E. (I believe), which, of course, kept a driving mist on the hill. The Sun seemed to lose as much power by obscuration as it gained by rising, and was powerless to disperse this, as it would otherwise have done, and our instruments were *dripping*. This I had thought possible in such a case, but I had hoped that the fine weather had set in, as such mist seems abnormal so late in the season. Very few minutes after totality the mist vanished from the returning heat.

Under these circumstances one might have thought photographs hopeless, and I went despondingly to ask for results. I found, to my great astonishment, that they had six apparently good ones, which is due to Hennessy, who exposed, having thrown over our programme, and increased his exposures, while Captain Waterhouse, who did the photographer's part ably, backed him by rapidly changing the slides.

A more careful examination than I could make before my telegram left, showed me that one of the photographs (the sixth) was imperfect, and, as luck would have it, this met with an injury by the removal of a portion of the film in the corona. The others are good. In all the Moon is elongated (from motion ?), but the rifts and radiated structure of the corona are clear, and the unchangeability seems, by a rapid examination, confirmed. For more than this I have not had time, as we got breakfast early, in hopes of getting all the instruments down-hill to-day, and I left soon after to my letters.

Herschel fitted up the Royal Society's 5-inch (in a way which he will, doubtless, describe) as an integrator, and instructed Col. Saxton in its use. I believe the reversion of the main lines was seen with it, but during totality only K 1474.

Ootacamund, Dec. 12, 1871.

Dr. Huggins has also received from Mr. Nursing Row, Fellow of the Society, a letter dated Dara Gardens, Vizagapatam, 13th Dec. 1871, containing an account of his observations of the Eclipse. The position of the Observatory is Lat. 17° 42′ 9″ N., Long. 83° 22′ 30″ E.; the computed times were—

	h	m	s
Beginning . . .	7	0	24
Greatest Phase . . .	8	1	36
End	9	6	2

the magnitude being ·629. The Eclipse was well seen, not a cloud in the vicinity of the Sun, and according to the observations the errors of the predictions were, for the beginning −12 seconds, for the greatest phase + 10 seconds, and for the end + 33 seconds.

After the greatest phase, the edge of the Moon's limb on the Sun's disk was observed, somewhat rough and uneven. At about six minutes to the end of the Eclipse this uneven edge attained a tinge of yellow colour, undulating. At the greatest phase the light of the Sun was dull, and on grassy grounds looked pale yellow.

A mean of the meteorological observations was forwarded, containing observations of the solar radiation and the humidity for every ten mimutes. The radiation observations were made with the solar-radiation thermometer, exposed freely to the Sun's rays in an open grassy place at the height of one foot from the ground, resting on two pieces of forked stick. The instrument was graduated on the stem, and had no attached scale, but enclosed in a large tube with a large bulb at one end, in the centre of which was the thermometer-bulb covered with lamp-black. The large tube was expanded with air and hermetically sealed.

There was also inclosed the copy of a sketch of the Sun's image previous to the beginning of the Eclipse, showing the positions of the spots then visible.

Meteorological Observations taken during the Time of the Eclipse, 12th December, 1871.

Hours. h m	Barometer.	Dry Bulb.	Wet Bulb.	Humidity.	Solar Radiation Thermometer.	Kind of Clouds.	Clear Sky.
7 1	30·138	73·5	67	69	40	ci	80
7 11	30·138	73·8	67	67	42	ci	85
7 21	30·142	74·3	67·5	68	46	ci	95
7 31	30·148	75·3	68·5	68	50·5	ci	95
7 41	30·154	75·6	68·5	68	50·5	ci	95
7 51	30·156	75·7	68·5	68	49	ci	95
8 1	30·160	75·8	68·7	68	48	ci	99
8 11	30·166	76	68·8	68	49	ci	90
8 21	30·170	76	68·8	68	51	ci	90
8 31	30·174	76·2	69	68	62	ci	90
8 41	30·180	76·5	69·1	68	68	ci	90
8 51	30·184	77	69·5	67	77	ci	90
9 1	30·192	77·2	69·8	68	82	ci	90
9 6	30·196	77·5	70	67	86	ci	88

On the Expression of M. Delaunay's $h + g$ in terms of his finally adopted Constants. By Prof. Cayley.

I had the pleasure of receiving from M. Delaunay a letter dated Paris, 17th Dec. 1871, in which he informs me that, on referring to his papers, he had found there expressions for l, g, h, identical with those given by me in the November Number of the *Monthly Notices*,—with only a single typographical error, $\frac{23}{33} e'^2 m^3$ instead of $\frac{23}{32} e'^2 m^3$ in my expression of h.

M. Delaunay mentions also that he had obtained four additional terms in the expression for $h + g$ (longitude of the Moon's perigee), and that the complete expression in terms of the finally adopted constants is —

$$h + g =$$

$$nt + \left\{\left(\frac{3}{4} - 6\gamma^2 - \frac{3}{8}e^2 + \frac{9}{8}e'^2 - \frac{45}{4}\gamma^4 + \frac{69}{8}\gamma^2 e^2 - 9\gamma^2 e'^2 - \frac{3}{32}e^4 - \frac{9}{16}e^2 e'^2 + \frac{45}{32}e'^4\right) m^2\right.$$

$$+ \left(\frac{225}{32} - \frac{189}{8}\gamma^2 - \frac{675}{64}e^2 + \frac{825}{32}e'^2 + \frac{1107}{16}\gamma^4 + \frac{81}{32}\gamma^2 e^2 - \frac{349}{4}\gamma^2 e'^2 - \frac{2475}{64}e^2 e'^2\right) m^3$$

$$+ \left(\frac{4071}{128} - \frac{3963}{32}\gamma^2 - \frac{31605}{512}e^2 + \frac{61179}{256}e'^2\right) m^4$$

$$+ \left(\frac{265493}{2048} - \frac{335403}{512}\gamma^2 - \frac{1483665}{4096}e^2 + \frac{1767849}{1024}e'^2\right) m^5$$

$$+ \left(\frac{12822631}{24576} - \frac{25291729}{16384}e^2\right) m^6$$

$$+ \left(\frac{1273925965}{589824} + \frac{352038855}{1179648}e^2\right) m^7$$

$$+ \frac{710286855589}{7077888} m^8$$

$$+ \frac{32145914707741}{679477248} m^9$$

$$\left. - \left[\frac{45}{32}m^2 + \frac{7425}{512}m^3\right]\frac{a^2}{a'^2}\right\}$$

On the Zodiacal Light. By Captain Tupman.

During a stay of some three years in the Mediterranean I had many opportunities of observing the Zodiacal Light. Occasionally, but very seldom, it was so bright and its boundaries so well defined, it was easy to trace the latter on a star-chart with considerable accuracy.

From the beginning these observations appeared to me to

present features of great interest, and I endeavoured to continue them on every favourable opportunity, especially in the early mornings when the eye-sight is most sensitive. After December, 1870, no such opportunity occurred, the planets *Venus* and *Jupiter* remaining for months in the middle of it, just when the ecliptic was most favourably elevated above the horizon.

The variation of its light is very remarkable. Sometimes it attains a brilliancy that is not sensibly affected by the brighter planets shining in the midst of it, and after an interval of a few days its light may be quite faint.

Its apparent angular extension from the Sun doubtless depends upon the keenness of the observer's sight, the clearness of the atmosphere, and the general inclination of its axis of symmetry to the horizon. An illustrious Continental astronomer once assured me that he frequently saw it completing the circle of the heavens, with a small cloud-like maximum of brilliancy opposite the Sun; and there are the well-known observations of M. Liais in corroboration.

The sight of a particular observer, however, may be considered constant, and, hence, great actual variations of the intensity of the light, not only near the Sun, but in every part of its zone, must be admitted.

Another, and perhaps more singular, feature is the variable position of the apparent axis of symmetry with regard to the plane of the ecliptic. It is true that this element cannot be determined with accuracy, but there are times when an error of 3 or 4 degrees is inadmissible. Such times have been chosen for making the sketches I now send, which show, not only an inclination of the axis to the ecliptic, amounting, in the months of August and September, to 20°, but also that *its plane does not pass through the Sun.* Six of the morning observations agree in placing the eastern node 40° behind the Sun, and the only evening observation places the western node 42° before the Sun; in the former the axis crossing from North to South, in the latter from South to North. The observation on the morning of the 7th of January, 1870, places the axis parallel to and 3° North of the ecliptic, and on that occasion an error of 3° in its absolute position was highly improbable, as 3° of azimuth on the horizon appears to cover a large space.

The effect of refraction in raising the lower part of the more inclined border more than the upper part is quite inappreciable.

It would be exceedingly interesting to compare observations of this nature made, simultaneously or nearly so, at considerable distances on either side of the equator.

In conclusion, I have only to remark that the drawings were made on charts containing the stars only; the ecliptic was then added and the whole traced off. I send the original tracings.*

* These tracings were exhibited at the November Meeting, and are preserved for inspection in the Library of the Society.

Details of the Observations.

Place.	Day (Astron.).	Time *before Sunrise*.	Inclination of Axis of Symmetry to the Ecliptic.	Distance of Node of Axis from the Sun.	Distance of Apex from the Sun, in Longitude.	Latitude of Apex.	Curvature of N. and S. Borders and general Angle between them.	Intensity of the Zodiacal Light.	State of the Atmosphere.	No. of Sketch.	Notes.
	1869.	h m	°		°	°					
Lisbon	Sept. 13	2 50	20	♈ 42	61	−6	Convex, 48°	v. v. bright	v. clear	1	The light fading off gradually at the borders. Axis almost coinciding with the parallel +18°.
,,	,,	1 50	20	♈ 42	51	−4	Convex, 38°	v. v. bright	v. clear	2	
,	Sept. 14	2 50	20	♈ 42	61	−6	Convex, 48°	v. v. bright	v. clear	−	The sketches of yesterday exactly suit the appearances to-day.
,,	,,	1 50	20	♈ 42	51	−4	Convex, 38°	v. v. bright	v. clear	−	
Cadiz	Sept. 15	2 0	20?	—	60?	—	Convex	v. bright	v. clear	−	Appearance the same as on two previous days.
Gibraltar	Oct. 4	2 0	0	—	66	+2	straight, 35°	bright	v. v. clear	3	Axis certainly not inclined 3°. An hour earlier the apex was observed in the same place, but the cone was narrower.
,,	Oct. 8	2 10	5	♌ 56±8	69	+2	straight, 34°	bright	v. clear	4	Borders sharply defined (comparatively).
Malta	Oct. 11	4 0 to 3 0	0	—	81	+2	straight, or slightly concave, 43°	v. v. bright	v. clear	5	The apex stationary among the stars for more than two hours, and more sharply defined than usual. Axis sensibly coinciding with ecliptic.
,,	Oct. 14	3 0 to	0	—	84	—	—	bright	v. clear	−	Apex beyond the Præsepe in *Cancer*, but after the brighter portion rose, it could not be traced so far.

,,	Nov. 3	1 30	0	—	69	—	Concave, ill defined, 26°	faint	v. clear	7	The light faint, although the air very clear.
,,	Nov. 11	2 20	0	—	77	—	—	bright	v. clear	—	Apex extending nearly to *Regulus*.
	1870.										
Malta	Jan. 7	3 10 to 2 10	0	—	113	+2	very concave	bright	v. clear	8	Axis parallel to, but 3° N. of ecliptic. At times the apex seen extending to β *Virg.* Base broad.
Vigo	Aug. 31	2 0	23	♍ 40	58	−6	Convex, 43°	bright	v. v. clear	9	The borders, if prolonged a little, would intersect at γ *Geminorum*.
,,	Sept. 5	1 40	16	♍ 40	73	−7	slightly concave, 20°	exceedingly bright	v. v. clear	10	The light exceedingly bright between *Venus* and *Mars*, both planets being within its cone.
Malta	Dec. 14	2 15 after sunset	7	♌ 42	160 to 170	—	slightly concave	bright	v. clear	11	*Evening*. Easily traced nearly to *Pleiades*, and at times 10°–20° beyond.
	1871.										
,,	April 29	2 0 before sunrise	—	—	—	—	—	—	—	—	No trace of it, although the ecliptic most favourably situated.
,,	May 7	1 40 after sunset	—	—	80	—	—	v. bright	v. clear	—	*Venus* and *Jupiter* both shining brightly in it. Axis about perpendicular to the horizon. Apex is in north latitude.
,,	May 8	2^h to 3^h after sunset	—	—	100	+4	—	faint	clear	—	Much diffused. After *Venus* and *Jupiter* had set, seen extending to α and γ *Leonis*.
,,	May 14	$1^h\ 50^m$ after sunset.	—	—	93	+4	—	bright	clear	—	Extending to α and γ *Leonis*. *Venus* and *Jupiter* shining brightly in it.

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, from 1871, *January to December.*

Communicated by the Astronomer Royal.

Occultations of Stars by the Moon.

Day of Observation. 1871-		Phenomenon.	Moon's Limb.	Mean Solar Time. h m s	Observer.
Jan. 31	(*a*)	Disapp. of *l* Tauri	Dark	4 48 34·5	J.C.
Mar. 3		Disapp. of η Cancri	Dark	11 9 18·4	H.C.
,,		Disapp. of 39 Cancri	Dark	15 1 13·2	,,
,,	(*b*)	Disapp. of 40 Cancri	Dark	15 4 52·6	,,
Mar. 28	(*c*)	Reapp. of μ Geminorum	Bright	10 53 31·7	E.
Oct. 23		Disapp. of τ^1 Aquarii	Dark	8 41 16·6	L.
,,		Disapp. of τ^2 Aquarii	Dark	9 49 29·7	,,
,,	(*d*)	Reapp. of τ^2 Aquarii	Bright	10 59 30·2	,,
Dec. 20		Disapp. of ν Piscium	Dark	12 49 32·1	J.C.

(*a*) Observed in daylight; the star was very faint, and the point of disappearance being very near the bright limb, the time noted may be a few seconds too soon.

(*b*) The star very faint; observation not satisfactory.

(*c*) The star was seen quite bright quite close to the limb.

(*d*) The time noted is probably correct to a second.

Phenomena of Jupiter's Satellites.

Day of Observation. 1871.		Satellite.	Phenomena.	Mean Solar Time of Obs. h m s	Mean Solar Time from N.A. h m s	Observer.
Jan. 5	(*a*)	I	Ecl. reapp.	6 11 57·9	6 11 48·4	C.
12		I	Ecl. reapp.	8 8 10·0	8 7 5·6	W.C.
17		I	Ecl. reapp.	15 33 34·3	15 33 34·8	,,
19		III	Tr. ing. 1st contact	7 43 57·4		J.C.
,,		III	,, bisection	7 48 56·5	7 47 ...	,,
,,		III	,, last contact	7 54 55·6		,,
26		I	Ecl. reapp.	11 57 49·3	11 58 1·8	C.
Feb. 6		III	Occ. reapp. 1st app.	7 8 7·9		J.C.
,,		III	,, bisection	7 12 37·1	7 16 ...	,,
,,		III	,, last cont.	7 17 6·4		,,
13		III	Occ. disapp. bisection	8 18 47·8	8 20 ...	E.
Mar. 10	(*b*)	III	Tr. ing. 1st contact	9 59 22·3	10 0 ...	C.
22	(*c*)	I	Ecl. reapp.	8 55 32·7	8 55 14·3	,,
April 15		III	Tr. egr. last cont.	9 44 48·9	9 41 ...	E.
Sept. 12		IV	Ecl. reapp	14 32 44·5	14 39 20·2	J.C.
Oct. 13		II	Ecl. disapp.	12 32 56·1	12 31 23·4	G.
22	(*d*)	III	Ecl. reapp.	12 11 51·6	12 12 28·4	W.C.

Day of Observation. 1871.	Satellite.	Phenomena.	Mean Solar Time of Obs. h m s	Mean Solar Time from N.A. h m s	Observer.
Nov. 5	III	Ecl. disapp.	17 0 49·1	16 58 29·9	G.
18 (*e*)	IV	Ecl. reapp.	15 6 23·2	15 12 27·0	W. C.
Dec. 4 (*f*)	III	Ecl. reapp.	12 7 55·8	12 5 47·5	G.
,,	III	Occ. disapp. 1st cont.	12 25 52·8	12 30 ...	,,
,,	III	,, last cont.	12 29 7·3		,,
29	III	Tr. ing. 1st cont.	12 16 36·4	12 19 ...	C.
,,	III	,, last cont.	12 23 5·3		,,
30	I	Ecl. disapp.	10 47 38·9	10 48 11·5	G.
,,	IV	Tr. ing. 1st cont.	11 7 45·6	11 21 ...	,,
,,	IV	,, last cont.	11 12 14·8		,,
,,	IV	Tr. egr. bisection	15 25 33·2	15 35 ...	E.
,,	IV	,, last cont.	15 32 32·1		,,

(*a*) Clouds passing over the planet.
(*b*) Cloudy; observation not satisfactory.
(*c*) The images of *Jupiter* and the satellites were very tremulous and faint.
(*d*) The time noted is very accurate. The satellite was of extreme faintness when first noticed.
(*e*) The satellite when first seen was extremely faint, and certainly was not visible 10^s before.
(*f*) Satellite very faint; it reappeared near the planet.

The initials W. C., E., C., L., J. C., H. C., and G., are those of Mr. Christie, Mr. Ellis, Mr. Criswick, Mr. Lynn, Mr. Carpenter, Mr. H. J. Carpenter, and Mr. Goldney.

An Early Transit of Mercury. Communicated by the Rev. A. Freeman, M.A., Fellow of St. John's College, Cambridge.

A transit of *Mercury* was observed at St. John's College, Cambridge, in 1782. The MS. record, never yet published, still exists. It appears to be in the handwriting of Dr. Isaac Pennington.

"Nov. 12, 1782.

Regulator Clock slow	m s 6 : 4·5	
Equation of Time	15 : 32 ‡	
Apparent Noon	XI^h : 38 : 22·5	per clock.
☿ Ingress	II : 51 :	apparent time.
Ingress	II : 29 :	per clock.
☿ Egress	IV : 15 :	apparent time.
Egress	III : 53 :	per clock.""

This observation is the more valuable, because clouds prevented Dr. Nevil Maskelyne from completing the observation at Greenwich. In the *Greenwich Observations* for 1782, he remarks, that, "On the going off of a cloud I first saw *Mercury* with the 46-inch

achromatic, magnifying 200 times, to make a considerable notch in the Sun's limb at $2^h\ 49^m\ 53^s$ apparent time, and at $2^h\ 54^m\ 55^s$ apparent time, I first was certain of light appearing between the limbs of the Sun and *Mercury*."

The mean of Maskelyne's observations would give a time $2^h\ 52^m\ 24^s$, which would be somewhat later than the ingress of *Mercury's* centre. Allowing 24^s for east longitude of Cambridge, this differs by 1^m from the record of St. John's College. The duration of the transit was $1^h\ 24^m$, remarkably long.

It should be noted at (‡) that the *Connaissance des Temps pour l'an* 1782 gives as the Equation of Time for the day $15^m\ 33^s$. This number of *seconds* is evidently required to make the sum of the first three times of the above record exactly equal to XII^h.

An account of the old Observatory of St. John's College, removed in 1859, will be found in the observations of the Rev. Thomas Catton, made between 1791 and 1826, and published in Vol. xxii. of the *Memoirs of the Royal Astronomical Society*, 1854.

Proposed devotion of an Observatory to observation of the phenomena of Jupiter's Satellites. By G. B. Airy, Astronomer Royal.

The position which the Royal Astronomical Society holds in the astronomical world may well justify it in employing its judgment and its influence in the direction of astronomical enterprises exterior to its own body. Guided by this consideration, I venture to submit to the Society whether they may not adopt, as an idea worthy of promulgation under their auspices, the proposal that one Observatory should be permanently devoted to the observation of the phenomena of *Jupiter's* satellites.

It is well known to the students of Gravitational Astronomy that the theory of the movements of *Jupiter's* satellites is a very singular one, perhaps the most interesting among the planetary applications of the theory of Gravitation. And the results are striking, especially in the remarkable enchainment which they exhibit in the movements of the three interior satellites, which at the same time are effected by the mass of the fourth. The fourth satellite has a claim peculiar to itself; it is on that satellite that observations must be made for determining the mass of *Jupiter* (the element which in the solar system is next in importance to the mass of the Sun). It is by it that the mass of *Jupiter* was determined originally (with some inaccuracy) by Pound, and subsequently in the first instance by myself (as is detailed in the *Memoirs* of this Society), and in the second instance by Bessel.

In late years the observations of the satellites have been

greatly neglected. The few observations that have been made of the third and fourth satellites are sufficient to show that the errors of their tables are too large to enable us practically to use these objects for even rude time-determinations, and at the same time are far too rarely determined to enable us to correct the elements of the tables. Of the first satellite there are, perhaps, sufficient observations, possibly also of the second satellite. But it must always be borne in mind that the tabular movements of the satellites cannot be corrected one by one; the necessary relation among their motions, to which I have already alluded, compels us to treat all together, and in combination with one of the most delicate theories of science.

The reason of the neglect to which I have alluded appears to be this, that the observations of such phenomena (occurring at what may be called irregular times, but times which are perfectly peremptory when the phenomena do occur) interfere most injuriously with the good order of the one class of observatories which has been strictly brought to rule, namely, the meridional observatories. And I see no prospect of obtaining a sufficient number of observations unless some observer would consent to give his entire observing energies to these observations. The accompanying observations for correction of clock would be of so light a character that I do not think them worthy of further notice.

It may be well to remark that, even to an observer who should determine to neglect no observation, a very fair amount of holiday would be left. Thus in 1872 there is no eclipse of satisfactory character visible at Greenwich between May 18 and September 18, and no other phenomena between June 9 and August 31. The interval between observations of less satisfactory character is from July 3 to August 29. In the winter oppositions of *Jupiter* the observations of satellites are rather pressing, for a time, but much less so in the summer oppositions.

The phenomena which first call for attention are undoubtedly the eclipses. Next to these, I am enabled to say on the authority of Laplace, are the transits of shadows upon the planet's disk, if they can be observed with sufficient accuracy. They possess this remarkable merit, that they are computed by the very same tables with which the eclipses are computed, without reference to the position of the Earth. Possibly the observation of transits of the satellites over the disk, and of occultations behind the disk, would be more accurate; they require, however, an additional calculation, depending on the position of the Earth. It would, however, be very desirable to examine all.

Having myself, in past years, repeatedly observed the phenomena of every class, and having at a more distant time studied the mechanical theory applicable to them, I may venture to express my belief that the mere observations, in their beauty and the incessant variation of their character, would be found very interesting; but that the highest interest would be felt on connecting them with their appropriate theory, and in making the

preparations for that numerical work by which each observation would be made to bear its part in the correction of elements and in the ultimate test of the theory.

Royal Observatory, Greenwich,
1871, *December* 26.

Remarks on the Planet Jupiter. By W. Lassell, Esq.

I send a sketch and a few remarks upon the planet *Jupiter*.

Now that the fourth satellite has begun again for a season to cross the planet's disk, I have looked out for opportunities of observing its transit, and was favoured on the night of the

30th Dec. last by witnessing a part of its passage over the disk under circumstances more than usually propitious.

On its first entrance it was scarcely to be distinguished from the limb, not appearing at all, as the others generally do, as a round, bright spot. As it advanced it grew gradually manifestly darker than the surface of the planet, and by the time it had advanced a fourth of the way across, it had become a very dark if not a *black* spot,—so dark indeed that if I had looked at

Jupiter without knowing any thing of the positions of his satellites, I should have said that a *shadow* was transiting. I remember having seen the like phenomenon many years ago; but my impression is that I had never seen the disk of the satellite so near to absolute blackness before. Of course it is only by contrast that it can possibly so appear; and we have in this fact a striking proof of the exceeding brilliancy of the surface of the planet. In the same way the solar spots, if not surrounded by the marvellous splendour of the Sun's surface, would doubtless appear as brilliant objects.

But this was not the phenomenon which struck me most in this rare and exquisite view of *Jupiter*. I acknowledge that I have hitherto been inclined to think that there might be some exaggeration in the *coloured* views I have lately seen of the planet; but this property of the disk in the view I am describing was so unmistakable, that my scepticism is at least beginning to yield.

I have attempted in the accompanying drawing, to represent the colours as faithfully as I can, and to convey something like a general notion of the distribution and intensity of the various lights and shades scattered over the planet; but to give any thing like a faithful outline of the individual phenomena so beautifully displayed on this occasion, is far more than I can pretend to. It is one of the advantages of a Newtonian reflector, when the alloy of the specula is well compounded, that colours of the planets are more faithfully represented than they can be by refracting telescopes, the want of perfect achromatism in the latter generally introducing some modifying tinge.

The picture is the result of the use of various powers,—260, 430, and 579, the last too high for the state of atmosphere,—and it was a doubtful point which of the others was to be preferred, though there were tranquil moments of the atmosphere when I thought 430 revealed the most. The aperture was 24 inches, which indeed I rarely diminish.

I may add further that the variety of colour is most obvious with the higher powers, and when the atmosphere will not permit their use the tints are scarcely apparent.

As the coloured drawing may present some difficulty of reproduction I inclose also a pencil drawing, with the names of the tints or colours opposite the respective parts, which may in some degree supply the place of the coloured drawing.

Ray Lodge, Maidenhead,
Jan. 2, 1872.

On the Probable Seat of Energy of the Eruption Prominences.
By A. C. Ranyard, Esq.

The enormous rate of ascent occasionally observed in the upheaval of eruption prominences—a most striking instance of which has been carefully discussed by Mr. Proctor in the last number of the *Monthly Notices*—leads us to inquire whether such vast velocities do not betoken something more than mere vertical storms in a gaseous envelope; or why the rate of motion observed in vertical storms or outrushes so greatly exceeds the velocities detected by the spectroscope in the horizontal currents. Further, have we any ground for conjecturing that the prominences are driven upwards by explosions taking place at great depths; or that they are carried outwards by solid masses hurled through the solar envelopes driving before them and followed after in their train by vast eddies of the vapours through which they pass?

When we consider the enormous temperature of the solar envelopes in contact with the photosphere, (estimated even by Zöllner at 68400° C., and calculated by Ericsson at 4035584° Fahr., and by Secchi at over ten million degrees Centigrade), it is at first difficult to conceive of matter existing in the Sun in any other state than that of vapour.

We are, however, so little acquainted with the behaviour of the elements under pressures and temperatures comparable with those which must exist at great depths within the solar globe, that it would be impossible to predict that there is no solid core to the Sun.* Prof. Young, speaking of the great prominence he observed in September last, says, "The whole phenomenon suggested most forcibly the idea of an explosion under the great prominence, acting mainly upwards, but also in all directions outwards." It is not necessary to conceive of chemical change in order that an explosion may take place; some alteration in the physical condition, some passage from the liquid to the gaseous state caused by an alteration of pressure—and involving no chemical change, may be sufficient to cause such an explosion, which, if it took place causing an expansion of the lower and denser vapours, would force them upwards into the outer and lighter envelopes, where, by reason of their greater specific gravity they would be carried further in the resisting medium than the lighter vapours composing the layer above them. Under such a supposition we might expect to find in the newly formed prominences their head or upper part composed of the denser vapours, and the lighter gases lagging behind. This, however, is the reverse of what is found upon the Sun. The prominences have their summits entirely composed of hydrogen, while they are often "gonflé"

* In other words, we cannot predict that the critical temperatures of some of the elements may not be even greater than solar temperature. See Prof. James Thompson's Paper before the British Association, Edinburgh.

with sodium and metallic vapours which have apparently been forced up into them from below, giving a structure such as we might expect to find if a series of layers had been disturbed by matter passing through them raising them up into prominences one inside the other.

The spiral twists which exist in the tall finger-shaped prominences do not form any insuperable objection to this theory of their eruption, for they may be caused after a very short lapse of time by horizontal vortices existing in the non-incandescent medium into which the prominences are projected.

It may be said that the existence of metallic vapours higher up than they are detected by the spectroscope is by no means improbable, and that because they have ceased to give out monochromatic light it does not follow that they are not there. To this it may be answered, that if such metallic vapours are there, and if they are at the same heat as the intensely incandescent hydrogen which forms the prominences, or, indeed, at the heat of a region of space so near to the photosphere, they would certainly be in a state of vapour; and if in a state of vapour, why not incandescent with their own wave length?

A theory has lately been started that meteors may be formed from the metallic vapours hurled from the Sun or other stars in their eruption prominences.

It appears, however, from the analysis of the Lenarto meteor given by the late Prof. Graham, and quoted by Mr. Proctor in his recent Paper on the "Corona," that the iron of which the meteor was chiefly composed contained at least three volumes of occluded hydrogen, pointing, as Prof. Graham believed, to its formation under great pressure in a dense mass of hydrogen such as we find in the chromosphere. Probably, however, there are no pressures in the chromosphere at all comparable even to atmospheric pressures, we are therefore forced to look to deeper levels (if we look to the Sun at all) as the laboratory where meteoric iron becomes solid, and to believe that the meteors were extruded from the Sun in a solid form* containing the hydrogen they at present hold occluded in them. These considerations taken in conjunction with the remarkable flashes across the chromospheric spectrum observed by Zöllner, Vogel, and Howlett in this country, appear to me to go far to render it probable that solid masses may be hurled from the Sun in eruption prominences.

* On this supposition meteorites would have very high, perhaps infinite, critical temperatures, an assumption surely less improbable than that we are forced to make on the theory that hydrogen is the exploding element, giving rise by its outrush to the prominences, in which case we must assume for it a critical temperature,—at least higher than the minimum solar temperature,—whereas hydrogen has not yet been reduced by pressure to the liquid state even at the lowest terrestrial temperatures. Of the twenty-two meteoric elements given by Miller, there are, however, others besides hydrogen which have not yet been reduced separately.

Description of a Printing Chronograph.

By G. W. Hough, Director of the Dudley Observatory.

About the year 1848 the idea of recording astronomical observations by the use of galvanic electricity was put in successful operation by different individuals. Since that time chronographs of various forms have been constructed for recording in a legible manner, on a moving sheet of paper, the time of any phenomenon observed. The great superiority, in point of accuracy and saving of labour over the old eye-and-ear method formerly used, led to the almost general adoption of the new plan.

During the past ten years the idea of constructing a chronograph which should print with type the time of the observation has been entertained by a number of persons. About five years since Prof. Hilgard, of the U.S. Coast Survey, made a description of an apparatus designed for this purpose, and about the same time Prof. C. A. Young, of Dartmouth College, published a proposed plan for one in *Silliman's Journal of Science*. But, so far as we are informed, the mechanical construction of such an apparatus has not heretofore been attempted by any one.

The construction of a machine which shall carry a type-wheel capable of given impressions, with uniform velocity for a number of hours together, without sensible variation in its motion, is a problem which is not easy of solution.

For a clear understanding of the mechanism elaborate drawings would be necessary, we shall therefore merely give a general account of its construction and peculiarities:—

1st. A system of clock-work carrying a type-wheel with fifty numbers on its rim, revolving once every second; one, two, or parts of two numbers being always printed, so that hundredths of seconds may be indicated. This train is primarily regulated to move uniformly by the Frauenhofer friction-balls, and secondarily by an electro-magnet acting on the fast-moving type-wheel, and controlled by the standard clock. This train is entirely independent, and can be stopped at pleasure without interfering with the other type-wheels.

2nd. A system of clock-work, consisting of two or more shafts, carrying the type-wheels indicating the minutes and seconds. The motion of this train is also governed by an electro-magnet, controlled by the standard clock, operating on an escapement in a manner analogous to the action of an ordinary clock, every motion of the escapement advancing the type one number.

There are three type-wheels, indicating minutes, seconds, and hundredths of seconds. The integer seconds are advanced at every oscillation of the standard pendulum, and the minute at the end of each complete revolution of the seconds-wheel.

Presuming now we have this system of type-wheels in operation, it is necessary to print without disturbing their motion; especially is this true for the fast-moving type-wheel.

After a long series of experiments, during which the fast-moving wheel was detached and stopped in various ways, the impression was finally made from the spring of the hammer only, not allowing the blow to fall directly on the type, but arresting it about half an inch before it reached the top of the type. By this device, which is regarded of the greatest importance, the motion of the type is not disturbed an appreciable amount. Any number of impressions following each other in rapid succession does not disturb the fast-moving wheel the one-hundredth part of a second. By this plan none of the type-wheels are stopped or locked in the act of printing, and records of observations may follow each other as fast as the hammer can be made to deliver the blow.

The printing may be done directly by means of a strong electro-magnet, but the cost and trouble of keeping up a large battery for this purpose led us to do all the work mechanically, only using electricity as the governing power. Accordingly, a heavy running gear was built for raising the hammer, capable in its present form of delivering 2000 blows without winding. This gearing is entirely detached from the hammer when elevated, but is unlocked just before the hammer reaches the type, immediately raising it again. The time consumed for this operation is about three-tenths of a second, allowing therefore observations to follow each other at a minimum interval of one-half second. When the hammer is elevated it is locked by an electro-magnet, the operation of this magnet allowing it to fall and print. As the hammer is acted on by gravity alone, the armature time will be sensibly uniform.

The types are inked by means of small rollers, covered with cloth, resting against their rim, and revolving with the wheel by friction. These rollers require inking every two or three days.

The paper fillet, two inches in width, is wound on a spool holding sixty feet, and drawn between two rollers the same as a Morse Register. Our spool of paper will hold about 1200 observations including the spacing for different objects.

Every time the hammer falls the fillet is advanced one quarter of an inch, by the action of an escapement driven by a weight. The same escapement is also operated by an electro-magnet under the control of the observer, who by pressing a key is able to make spaces of any width between the prints.

To recapitulate, the following are the distinctive features of this mechanism :—

1st. Separate movements for the integer seconds and the hundredths of seconds; 2nd. The method of regulating the hundredths of seconds wheel by an electro-magnet in connection with the standard clock; 3rd. The method of printing double or single numbers without stopping the type-wheels; 4th. The method of striking the blow, indirectly using the spring of the hammer; 5th. The method of elevating and locking the hammer.

The battery power required is about the same as for an ordinary chronograph. Three Grove elements or six Hill's elements

work the two electro-magnets well. A separate battery of about the same size is used for the hammer and fillet magnets.

In point of accuracy this machine leaves nothing to be desired, and is much beyond what we thought possible. From a vast number of experiments, made by recording automatically the beats of the standard clock, both at the middle and end of the oscillation, the mean error for a single print is found to be $0^s{\cdot}013$, equal in this respect to the recording chronograph. The maximum difference in the records of the beats seldom exceeds $0^s{\cdot}03$, and we believe this is as much due to the irregularity in the clock connexion, as in the running of the machine, since the same thing is found in ordinary chronographic records, where the measures are made from second to second.

The saving of time and labour by the use of a printing chronograph is very considerable. At the lowest estimate it does work equivalent to the labour of one person where three are employed at the same time. In our zone-work in former years, when the zone extended two hours in right ascension, it usually required the labour of two persons a whole day to convert the chronograph records into numbers and copy them on the blank forms. With the observations printed this labour is wholly dispensed with, since the "mean" is at once deduced from the printed records.

The machine is readily adjusted to indicate the same numbers as the clock's face, the type being so set as to print zero-hundredths when the pendulum is at its lowest point, where the magnetic circuit is completed.

In the observation of zone-stars or regular transit-work the type may be set to give the integer seconds of mean right ascension, so that the final reduction will always be a small quantity.

The constant use of this mechanism on every day and observing for six months, during which time more than twelve thousand records have been made, enables us to speak with confidence of its success, both as regards correctness in printing and in saving of labour.

Other things being equal it is found that for three observers twice as many observations can be reduced in the same time as when a recording chronograph is employed.

Formulæ for the Calculation of the Orbit of a Double Star.

By M. Annibal de Gasparis. (Translation.)

Having reduced by Herschel's method (*Memoirs R.A.S.* vol. v.) positions for six equidistant epochs, let $\varrho_1, \varrho_2 \ldots \varrho_6$ be the distances and $\phi_1, \phi_2 \ldots \phi_6$ the angles of position. Denoting by m_{12} the triangular area described on the plane of projection, and com-

prised between the distances ϱ_r and ϱ_s, we have $m_{rs} = \frac{1}{2}\varrho_r \varrho_s \times \sin(\phi_r - \phi_s)$. This being so, developing the area m_{rs} described on the plane of the orbit as a function of the radius vector and its derivatives, including the terms multiplied by the sixth powers of the times, we have to determine the ratios of the radius vectors r_2, r_3, r_4 the two equations,—

$$\frac{r_2^3}{r_3^3} = \frac{42\,m_{23} - 50\,m_{34} - 6\,m_{45} - 18\,m_{24} + 13\,m_{35} - 11\,m_{46} + 40\,m_{56} + 2\,m_{25}}{42\,m_{12} - 50\,m_{23} - 6\,m_{34} - 18\,m_{13} + 13\,m_{24} - 11\,m_{35} + 40\,m_{45} + 2\,m_{14}},$$

$$\frac{r_3^3}{r_4^3} = \frac{42\,m_{45} - 50\,m_{34} - 6\,m_{23} - 18\,m_{35} + 13\,m_{24} - 11\,m_{13} + 40\,m_{12} + 2\,m_{25}}{42\,m_{23} - 50\,m_{34} - 6\,m_{45} - 18\,m_{24} + 13\,m_{35} - 11\,m_{46} + 40\,m_{56} + 2m_{25}}.$$

These ratios being known, the direct calculation of the elements is quickly effected, since in fact the numerators and denominators are quantities of the third order. For this reason I thought it proper to use six positions (instead of four, which would have been sufficient) to attenuate in Herschel's curve the errors of observation, making use of a larger number of data. The memoir containing the necessary developments and other details will appear in vol. vi. of the *Atti* of the Academy of Sciences of Naples.

Occultation of Vesta, Dec. 30, 1871. By C. G. Talmage, Esq.

The occultation of *Vesta* was well seen here on the 30th Dec. The day had been very wet, but by 9·30 P.M. the sky was quite clear. *Vesta* was exceedingly bright right up to the Moon's limb.

The G.M.T. of immersion was

$$10^h\ 44^m\ 24^s{\cdot}8$$

and of emersion

$$11^h\ 52^m\ 52^s{\cdot}6.$$

Both exact. Power 80 on 10-inch refractor.

Note on the Variable Star S *Orionis.* By the Rev. T. W. Webb.

I have taken the earliest opportunity of reobserving the little variable star S *Orionis*, the character of whose light, suspected early in the year 1870, I was so fortunate to ascertain satisfactorily about this time twelve months ago. I find that it is now considerably smaller than on any previous occasion when I have looked for it; and as it is well situated among comparison-stars,

whose magnitude has been carefully determined by Mr. Baxendell, the investigation of its changes becomes an easy matter.

It may be remembered by those who have examined it, that it has a south-preceding companion, *d* of Baxendell, 11·1 mag.; and another north-preceding, *e*, 11·5 mag.; while at a greater distance we find *f*, nearly following, 11·6 mag.; *g*, north, 11·7 mag.; and two minuter points *h*, south-following, 12·5 mag.; and *k*, just beyond *e*, 12·6 mag. On the 5th instant I found that it was much fainter than its nearest neighbours *d* and *e*, and smaller than *f* and *g*, the lesser of which has 11·7 mag., but brighter than *h* and *k*, the larger of which is rated 12·5 mag.; but as it was nearer *g* than *h*, it may be estimated as a little brighter than 12 mag. Taken altogether, the observations point to a period of less than twelve months; but further examination is obviously required; and whether the star may even now have reached its *minimum* is uncertain.

Hardwick Vicarage, 12 *Dec.* 1871.

On the Identity of the Triple Star H. i. 13.

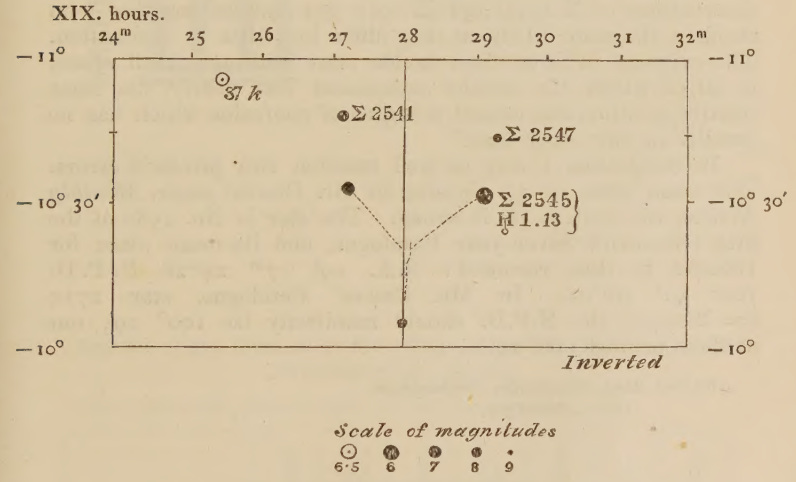
By George Hunt, Esq.

In the *Monthly Notices*, November, 1862, the late lamented Mr. Dawes wrote a paper with the above title, on a triple star in *Aquila*. The perusal of that memoir leaves, I think it will be admitted, no doubt on the mind of the reader, that Mr. Dawes has clearly shown the identity of H. i. 13 with Σ 2545; yet it has always seemed to me, that he has not sufficiently dwelt upon *one* item in Sir W. Herschel's description of his star, which appears to establish the identity beyond all question. Sir W. Herschel says, "It is the last star of a telescopic trifolium *n* following *k*, similar to that in the hand of *Aquarius*."

In the summer of 1868 I examined this region carefully with an Equatoreal of 4-inch aperture, by Simms, and soon made out Struve's three stars Nos. 2541, 2545, and 2547, but was at first much puzzled to identify Herschel's *trifolium*, until I put on a comet eye-piece magnifying twenty-seven times with a very wide field, when the beautiful trifolium was at once visible, having Struve's No. 2545 as "the last star." It occurred to me it would be interesting to map down to scale, all the stars closely north following 37 (*k*) *Aquilæ*, which could be found in Weisse's Bessel's Catalogue, and I venture to bring the result before this Society, as in some sort supplementary to Mr. Dawes' paper. In the accompanying small map, the stars are all inserted from Weisse's Catalogue, with the exception of Struve's 2545 and 2547, which are inserted from Struve's *Positiones Mediæ* (corrected for precession to 1825), and as my object is merely to show the *relative* position of the neighbouring stars, I have not applied the pre-

cession in R.A. and Dec. to the places of Weisse's Catalogue, of which the epoch is 1825. The *trifolium* is distinguished by the dotted lines. The stars in Weisse's Bessel included in the map, are, in the order of their R.A., *Hora* xix, No. 677 (= 37 *Aquilæ*), 692, 719 (= Σ 2541), 722, 744, 745, of which the last three and Σ 2545 or H. i. 13 constitute the *trifolium*. Struve's stars Nos. 2545, 2547, are not contained in Weisse's Bessel.

Mr. Dawes, in the paper above mentioned, after remarking that neither South, nor Struve, nor Herschel II, nor Smyth, saw the third star of this H. i. 13., proceeds to say that he saw it on August 19th, 1862, with a 4-inch aperture object-glass, by Cooke, "with so low a power as 135." This I can confirm with my own 4-inch Equatoreal.



In July, 1868, I find the following was inserted in my Note-book :—"Σ 2545. Best shown double with 120, third star glimpsed by averted vision, also with 200." In order to put it beyond doubt that the third star was actually seen, the same night I used Mr. Dawes' Solar eye-piece with a very small field and power 260, so as to exclude the close pair ; and under these circumstances the third star was (to use Admiral Smyth's phrase) "staring."

I cannot at all account for the R.A. and Dec. given in the Cycle under the numbers 702 and 703. While the description, "the three lie nearly in a line," shows conclusively that the worthy Admiral had his telescope directed to Σ 2547, the R.A. and Dec. given is exactly that of Struve's 2541 in the *Positiones Mediæ* for 1830, corrected for ten years' precession to bring it up to Smyth's Epoch for 1840, Σ 2547 having more than

2^m greater R.A. (This has a bearing on one of Sir John Herschel's queries, in the last *Monthly Notice* for Nov. 1871.) The R.A. of 703 does not seem to be that of any of Struve's three stars, the Dec. coinciding with that of Σ 2545 or H. I. 13. On the supposition that Smyth's 702 = Σ 2547, there is yet another difficulty not noticed by Mr. Dawes in his paper of Nov. 1862. Admiral Smyth gives the distance of the close pair = 3″·2, while Mr. Dawes gives an approximative measure = 20″±, and Struve, as the mean of three sets of measures, = 20″·70. Epoch 1830·02. Mr. Dawes, in his long and interesting note on this subject, in his Double-Star Catalogue, pp. 494-496, suggests a plausible explanation of these discrepancies. He says, "It is true that his (Smyth's) estimations of the distances of the stars composing Σ 2547 are very far from correct; but it seems highly probable that by some means his observations of Σ 2541 and Σ 2547 got mingled together—an accident the more likely, as they differ but little in declination. The existence of three close double stars within a small space, in all of which the smaller component has nearly the same relative position, has caused a degree of confusion which has no parallel in any other case."

In conclusion, I may as well mention two printer's errors. The mean place of 37 *Aquilæ*, in Mr. Dawes' paper, *Monthly Notices*, vol. xxii. p. 36, is wrong. The star is No. 1580 of the first Greenwich Seven-year Catalogue, and its mean place for 1860·00 is thus recorded: R.A. $19^h\ 27^m\ 24^s·28$ N.P.D. 100° 51′ 46″·04. In Mr. Dawes' Catalogue, star 2715 (= Σ 2545), the N.P.D. should manifestly be 100° 29′, not as there printed 110° 29′.

16 *Chad Road, Edgbaston, Birmingham,*
1872, *January* 3.

Note on the November Meteors. By Capt. Noble.

Watching almost persistently on the night of Monday, November 13, from $12^h\ 5^m$ to $13^h\ 15^m$, I failed to see a single meteor of any sort or description. The succeeding night was densely cloudy.